

The philosophy of economics

An anthology

SECOND EDITION

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Introduction

Premises assumed without evidence, or in spite of it; and conclusions drawn from them so logically, that they must necessarily be erroneous.

– Thomas Love Peacock, *Crotchet Castle*

Ever since its eighteenth-century inception, the science of economics has been methodologically controversial. Even during its period of greatest prestige (the first half of the nineteenth century) there were skeptics, most of whom were less amusing than Peacock. Economics is certainly a peculiar science. Many of its premises are platitudes such as: Individuals can rank options, or individuals prefer more goods to fewer, or individuals choose that option that they most prefer. Other premises are simplifications such as: Commodities are infinitely divisible, or individuals have perfect information. Upon such platitudes and simplifications, such “premises assumed without evidence, or in spite of it,” economists have erected a large theoretical edifice. This edifice possesses immense mathematical sophistication, but its conclusions, though not “necessarily erroneous,” are often inapplicable. Can such an enterprise be a science?

This is an ancient, complex, and obscure question, since it is by no means obvious what is meant by asserting (or denying) that economics is a science. To be called a science is, no doubt, an honor. As the scientific credentials of economics rise, so do consulting fees. But the question Is it a science? is multiply ambiguous.¹ Is one inquiring about the goals of the enterprise, about the methods employed, about the conceptual structure of the theory, or about the extent to which the discipline can be united to, or reduced to, physics? In asserting that economics is or is not a science, is one necessarily asserting that it is the same *kind* of science as are the natural sciences, or might the social sciences be a species of science different from that of the natural sciences?

Although these questions are not always disentangled, they have bothered and occupied philosophers and economists for the last two centuries, and they remain live questions today. In fact, during the last generation there has been a resurgence of interest in philosophical and methodological questions concerning economics. This new interest is broadly based. Philosophers, economists, other social scientists, and ordinary citizens have all felt more need to understand what sort of an intellectual discipline economics is and what sort of credence its claims merit.

One of the major reasons for this increased interest is that economies

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are not performing as well as they used to. In the late 1960s many economists believed that the problems of regulating the overall performance of modern “free-enterprise” economies had been solved. The performance of “developed” economies during the two decades following World War II was superior to any period in the past. There was some unemployment and inflation, but the problems seemed soluble. Confidence in accepted economic theory rose to a peak in the 1960s.

Looking at the economies of various nations now (the spring of 1993), one finds not only a much gloomier prospect, but also widespread doubt that *anybody* knows how to restore prosperity without aggravating budget deficits, to reinstitute markets in state-controlled economies without precipitating economic collapse, or to alleviate the continuing and indeed increasing misery characteristic of most of the so-called “developing” countries. Not only does the lay population doubt economists, but economists doubt themselves. In such an atmosphere it is not surprising that economists should turn to methodological reflection in the hope of finding some flaw in previous economic study or, more positively, some new methodological directive that will better guide their work in the future. And it is even less surprising that ordinary citizens, whose opinions of economists are more influenced by the state of the economy than by any systematic evaluation of economic theories, should wonder whether there might be something fundamentally awry with the discipline.

There are, moreover, three important theoretical reasons, independent of any crisis in economics, why methodological questions concerning economics should be of greater interest now. First, not only economists, but also anthropologists, political scientists, social psychologists, and sociologists influenced by economists have recently argued that the “economic approach” is the only legitimate or fruitful approach to the study of human behavior.² They have, that is, argued that economics is the model that *all* social scientists must follow. This is a provocative claim, which makes methodological questions concerning economics more directly significant to practitioners of other social sciences.

Of course, the interest of other social scientists in the methodology of economics is not brand new. Many questions one might ask about the methodology of economics also concern other social sciences. Those who wonder, for example, whether there can be any laws of human behavior cannot help being interested in whether economists have in fact formulated such laws. As the most “advanced” of the social sciences, economics is bound to be of interest to those concerned with the nature and possibility of any “science” of human behavior.

It is ironic that various economists and other social scientists have been making grandiose claims for the universal validity of the economic ap-

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proach to human behavior at just that time when so many economists have had qualms about their own discipline. But there is a further ironic twist, which provides the second theoretical reason why interest in the methodology of economics is increasing. During the same period that grand claims have been made for the economic approach to human behavior, cognitive psychologists and economists impressed by the work of cognitive psychologists have been subjecting fundamental claims of modern mainstream economics to stringent psychological testing.³ The results are still ambiguous, but they call many of these claims into question.

Finally, there are special reasons why philosophers are becoming more interested in the methodology of economics. Contemporary philosophers have grown skeptical of received "wisdom" in the philosophy of science, and they have become convinced that there is a great deal to be learned about how science *ought* to be done from studying how science *is* actually done. Philosophers of science are thus undertaking more systematic and detailed studies of the history of science and of the state of contemporary sciences than was the case a generation ago. Although most of this work has focused on the natural sciences, philosophical attention to economics has been growing, since economics is, in fact, a particularly interesting science for a philosopher to study. Not only does it possess the aforementioned methodological peculiarities, but moral philosophers, whether attracted or repelled by the tools and perspectives provided by economists, have been forced to come to terms with the achievements and failures of welfare economics.

This renewed interest in economic methodology comes after decades during which the subject was largely ignored by philosophers, whereas the efforts of economists – in many cases prominent ones – were sporadic and often as polemical as they were philosophical. Much of the literature on the methodology of economics is unsure of itself and uninformed about the history of philosophical discussion of economics. This anthology can help remedy this state of affairs. There is, I believe, a great deal to be learned from studying directly how intellectual giants such as Mill or Weber or Marx attempted to deal with the methodological problems of economics.

Although this anthology permits various writers on the methodology of economics to speak for themselves, some introductory material may help the reader to understand and to appreciate the various essays. In the remainder of this introduction, I shall provide some general background to make the essays more accessible and to help avoid misunderstanding. Capsule introductions to the philosophy of science, to economic theory, and to the history and contemporary directions of work on economic methodology follow. With the help of these introductory materials, the

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selections in this anthology should be useful to readers with no special expertise in either philosophy or economics.

An introduction to philosophy of science

Science is one sort of human cognitive enterprise, and philosophy of science is consequently a part of epistemology (the theory of knowledge), although philosophers of science also face questions concerning logic, metaphysics, and even ethics and aesthetics. One can find discussions of issues in the philosophy of science in the works of pre-Socratic philosophers, but philosophy of science as a recognizable subspecialty only emerged gradually during the last two centuries. Important names in the early development of modern philosophy of science are David Hume and Immanuel Kant in the eighteenth century and John Stuart Mill and William Whewell in the nineteenth century. Only at the end of the nineteenth century were an appreciable number of monographs specifically devoted to philosophy of science written, and these were mainly by scientists or historians of science (figures like Ernst Mach, Pierre Duhem, and Henri Poincaré) rather than by professional philosophers. In the first half of the twentieth century, the so-called logical positivists (many of whom also had backgrounds in science) dominated philosophical thinking about science,⁴ although Karl Popper's views also exerted a considerable and growing influence. Contemporary philosophy of science is an area of lively research and controversy. Although there is some agreement about how to study philosophical questions about science, there is little agreement about what the answers, or even the most important questions, are.

The various issues with which the philosophy of science has been concerned can, I think, usefully be divided into six groups:

1. What are the goals of science? Is science primarily a practical activity that aims to discover generalizations that will be useful, or should science seek explanations and truth?
2. What is a scientific explanation?
3. What is a scientific theory and what are scientific laws? How are theories related to laws? Why are theories important in science and how are they used by scientists? How are scientific theories and laws discovered or constructed?
4. How are theoretical claims related to observations? How can we have knowledge based on observation and experimentation of entities like electrons or neutrinos, which cannot be observed? Should claims about unobservables be taken as literally true or false or as

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useful fictions that enable scientists to make accurate predictions concerning things that are observable?

5. How does one test and confirm or disconfirm scientific laws or theories and how can one distinguish them from the claims of other disciplines? What are the differences between the attitudes and practices of scientists and those of members of other disciplines?
6. Are the answers to the preceding five questions the same for all parts of all sciences at all times? How does the way that science is “done” differ among different sciences and how does it develop and change? Can human actions and institutions be studied in the same way that one studies nature?

Philosophers of science have been concerned with many other issues, too. A great deal of contemporary work focuses on detailed conceptual and epistemological questions raised by specific sciences. Problems such as those raised by modern physics about space and time or about indeterminism are continually being brought to philosophy or stolen from it by advances in the sciences. As the work collected in this anthology illustrates, a good deal of philosophy of science is concerned with the interpretation of specific scientific work and differs only in emphasis from the work of theoretical scientists.

In discussing these six sets of questions I shall present not only contemporary views, but also their positivist and Popperian ancestors. For contemporary philosophy of science is in large part a reaction against the views of Karl Popper or of the logical positivists and cannot be understood very well except against the background of positivist and Popperian philosophy of science. And, in any case, logical positivist and particularly Popperian views are still influential among economists.

The goals of science

There have traditionally been two main schools of thought concerning the goals of science. So-called scientific realists have held that science should not only enable us to make accurate and reliable predictions, but that it should *also* enable us to discover new *truths* about the world and to *explain* phenomena. When a theory is well supported, the realist holds that one can regard its claims, even those that talk about unobservable things, as true, although almost all realists concede that the findings of science are *corrigible*, that is, subject to correction with the growth of science. Members of the other school, so-called instrumentalists, have been more reserved about whether one can regard the claims that theories make about unobservables as true. Instrumentalists insist that the goal of

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science is the development of tools that enable one to make reliable and useful predictions. Some instrumentalists have laid great stress on the *practical* importance of scientific predictions, whereas others have simply been more suspicious of the possibility of finding the truth and of giving theoretical explanations. Notice that realists and instrumentalists *agree* that scientists should develop theories that apparently talk about unobservable things and properties. They disagree about the goals of science and about the interpretation of claims about unobservable things.⁵ In his influential essay, "The Methodology of Positive Economics," reprinted here, Milton Friedman espouses an instrumentalist view of science.

Who is right? Should scientists confine themselves to forging tools that enable us to make accurate and reliable predictions and thus to build airplanes and computers, or should they aim "higher" at the *truth* about nature and society? There is no settled opinion among either philosophers or scientists. Realism has a firm foothold in most areas, but the problems and peculiarities of quantum mechanics have led many physicists to a modest view of the goals of science.

It should not, by the way, be thought that someone who hopes that science can discover new truths about the world through its theorizing must find theories valueless unless they are true. Ptolemy's astronomy, which places the earth in the center of the solar system, is still used for navigational purposes even though it is, of course, full of false claims. There is no reason why the realist cannot use Ptolemy's theory to navigate, too. The realist wants more from science than merely useful theories, but he or she can value and employ such theories nevertheless. The realist can, of course, also recognize that all the engineer or policy maker needs in a scientific theory is a source of reliable predictions.

Scientific explanation

Explanations remove puzzlement and provide understanding. Often people think of explanations as a way of making unfamiliar phenomena familiar, but in fact explanations often talk about things that are much *less* familiar than what is being explained. What could be more familiar than the fact that water is liquid at room temperature? Certainly not the explanation physicists give for its liquidity.

Many philosophers have argued that the core idea in a scientific explanation is that it shows some happening or some regularity to be an instance of a broader or "deeper" regularity. Where there was contingency and multiplicity, science shows the unity of an underlying regularity. Notice that in explaining something by revealing it to be an instance of a more fundamental law, one need not have any explanation for that

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law itself. Explanations always come to an end at the frontiers of science, which is not, of course, to say that those frontiers cannot be extended.

This notion of explanation goes back to the Greeks, but Carl Hempel provides the most systematic exposition of it.⁶ Although Hempel was a logical positivist when he began his work on explanation, this basic notion of explanation not only antedates the positivists, but it has survived logical positivism's collapse. Hempel develops two main models of scientific explanation, the deductive-nomological and the inductive-statistical models. The latter, as its name suggests, is concerned with statistical explanations and attempts to extend the basic intuition of the deductive-nomological model. Statistical explanations raise a host of serious difficulties, and my focus will be on Hempel's account of nonstatistical explanations, his deductive-nomological or D-N model.

In a deductive-nomological explanation, a statement of what is to be explained is *deduced* from a set of *true* statements, which includes *essentially* at least one *law*. Schematically, one has:

True statements of initial conditions

Laws

Statement of what is to be explained

where the line represents a deductive inference. One shows the particular happening or regularity to be explained to be an instance of some broader regularities by deducing a statement of what is to be explained from those broader regularities and other true statements. For example, one explains why so many more computers are sold today than were sold five years ago by deducing this fact from the "initial condition" that computers are much cheaper now and the "law" that more of any commodity will be purchased when its price is lower. The presence of at least one *law* in a deductive-*nomological* explanation is essential. To deduce that this apple is red from the true generalization that all apples in Bill's basket are red and the true statement that this apple is in Bill's basket does not explain *why* the apple is red. "Accidental generalizations," unlike laws, do not enable us to explain phenomena.

Furthermore, to have truly explained some phenomena, one must be able to deduce it from a set of *true* statements.⁷ If one *believes* that the statements in a purported explanation are true (and that all the other conditions are satisfied), one will *believe* that one has given a good explanation, but one has only succeeded in fact when the statements one makes are true.

Recall that the D-N model is only an account of deterministic explanations. If one has only a statistical regularity, then one will not be able to

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deduce what is to be explained, but one may be able to show that it is highly probable, which is what Hempel's inductive-statistical model requires.

Not only is the D-N model limited to nonstatistical explanations, but it also does not give sufficient conditions for something to be a scientific explanation. An argument may satisfy all the conditions of the D-N model without being an explanation. Consider the following example:

Nobody who takes birth control pills as directed gets pregnant.
George takes birth control pills as directed.

George does not get pregnant.⁸

If George is a man, nobody would regard this argument as explaining why George does not get pregnant. If we assume for the sake of this discussion that the first premise is a law and that George does faithfully take his birth control pills, then all the conditions of the D-N model are met, but one still does not have an explanation.

Why not? The intuitive answer is that it does not matter whether George took birth control pills. His taking the pills was not causally relevant to whether he got pregnant or not.

To explain apparently requires not only that the factors one cites be causally relevant but also that they be *causes*, rather than effects or effects of a common cause. For example, knowing the angle of elevation of the sun, one can deduce the height of a flagpole from the length of its shadow or vice versa.⁹ Both deductions satisfy the conditions of the D-N model, but only the deduction of the length of the shadow counts as an explanation. Such explanatory asymmetries point to the importance of causation: Causes explain their effects, but effects do not explain their causes, and effects of a common cause do not explain one another.

Given these facts about explanation and an increased appreciation of the general importance of causal notions in the sciences, philosophers such as Nancy Cartwright, David Lewis, Richard Miller, and Wesley Salmon have argued that one should abandon the D-N model and develop the intuition that explanations cite causes of the phenomena to be explained.¹⁰

One might still regard the D-N model as providing necessary conditions for nonstatistical explanations. But even this weaker thesis requires qualifications, for explanations in science rarely fit the deductive-nomological model explicitly. Defenders of the D-N model respond by arguing that actual scientific explanations are often elliptical or mere explanation sketches. Even if one accepts these excuses, the D-N model seems to abstract significant facts concerning context and causality.

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Many philosophers would still maintain that the D-N model is an important starting point for studying scientific explanation and that there is something right and important about it. But here, as elsewhere, many have argued that a completely abstract approach cannot succeed and that there is more to be learned by examining the specific demands on scientific explanations that scientists committed to particular theories or research approaches have made.

The explanation of human behavior in economics or elsewhere introduces special difficulties. Most explanations of human action take one simple form. One explains why an agent made a phone call or purchased some stock or changed jobs by citing the relevant beliefs and desires of the agent (though one rarely mentions explicitly *all* the relevant beliefs and desires). Although economists are not interested in the details of an individual's behavior, their explanations of choices in terms of beliefs (or expectations) and utilities (preferences) have just the same form.

This familiar kind of explanation is philosophically problematic. If one attempts to construe such explanations as sketchy deductive-nomological explanations, one finds that it is hard to formulate any substantial laws that are implicit in them. One winds up with platitudes like, "People do what they most prefer." Some philosophers have argued that generalizations like these are not empirical generalizations (scientific laws) at all, but that they are implicit in the very concepts of action and preference.¹¹ According to these philosophers, explanations of human behavior (in terms of the *reasons* why agents act) differ decisively from explanations in the natural sciences. These philosophers argue that reasons are not contingently connected to actions, as causes are to effects, but that an agent's reasons serve to *define* what an agent's action is. To explain why an agent pushed certain buttons on a small black box by pointing out that the agent wanted to multiply 27 by 39 explains the action by saying what the action is. It does not seem that one is subsuming the action under any causal regularity. By giving the agent's reasons, one elucidates the nature of the action.

It is true that in explaining an action, one gives the agent's reasons for performing it. Beliefs and desires function as reasons for action. But is it true that explanations in terms of reasons differ fundamentally from explanations in the natural sciences? Perhaps explanations in terms of reasons might *also* be (roughly) deductive-nomological explanations in terms of causes.¹² Can they not be tested and assessed in more or less the same way that explanations in the natural sciences are assessed? Philosophers disagree sharply on these questions. Most writers on economics have regarded explanations in economics as, in principle, of the same variety as explanations in the natural sciences, but there are some promi-

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nent dissenters, such as Frank Knight or members of the modern “Austrian” school.

Scientific theories and laws

Most philosophers have recognized that science proceeds by the discovery of theories and laws. Philosophers have, however, differed considerably concerning what theories and laws are, how they are related, and why they are important. Let me begin with a few words about laws.

The laws that scientists seek to establish are not prescriptive laws dictating how things *ought* to be but expressions of regularities in nature. It is not as if the moon would like to leave its orbit around the earth, but is forbidden to do so by some law of gravitation. Natural laws are true expressions of regularities.

It is, however, difficult to explain how laws differ from accidental generalizations. As we saw before, explanations require genuine laws. Accidental generalizations will not do. It seems easy to distinguish a generalization such as “All the candies in the jar are orange” from a law, because this generalization, unlike any fundamental law, is restricted to a particular place and time. But consider the generalization, “All buildings are less than 10,000 stories tall.”¹³ This generalization is not only well supported by what we know of past history, but it might well be true of the whole universe throughout the whole of time. Yet most people would regard it as merely an accidental generalization and not a law. Why?

The obvious answers raise serious philosophical problems. Many people would be inclined to say that laws state what *must be*, not merely what happens to be. Since it is *possible* that a building more than 10,000 stories tall be built, the claim that none are so tall cannot be a law. But it is hard to understand the concept invoked here of a “physical necessity” or of a “necessity in nature.” Furthermore, if laws express necessities, then one wonders how one can get evidence of their truth. If all evidence comes, however indirectly, from observations and experiments, how can one distinguish universal generalizations that happen to be true from universal generalizations that must be true?

A second way that one might attempt to distinguish laws from accidental generalizations runs into similar problems. Laws appear to support counterfactual conditionals, whereas accidental generalizations do not. A law such as “Copper conducts electricity” supports conditional claims such as “If this wood pencil were made of copper, it would conduct electricity. An accidental generalization such as “All the candies in the jar are orange” does not, on the other hand, support claims such as “If this chocolate were in the jar, it would be orange.” But the empirical assess-

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ment of counterfactual claims has proven to be just as difficult as the empirical assessment of claims about necessities.

One less metaphysical difference between laws and accidental generalizations is that laws are supported by, incorporated in, or derivable from accepted scientific theories, whereas accidental generalizations are not. Suppose somebody demonstrated that our theories of matter implied that buildings must always collapse before they could be made 10,000 stories tall. In that case we would cease to regard the generalization that all buildings are fewer than 10,000 stories high as merely accidental. We would instead regard it as a derivative and uninteresting law. So an attempt to understand what laws are leads one to attempt to understand what scientific theories are.

But it is not any easier to understand what a scientific theory is than to understand what a law is. Theories appear to be collections of lawlike statements¹⁴ that are systematically related to one another. But if in order to understand what theories are, one needs to know what laws are, then one cannot rely on the connection between laws and theories to analyze the notion of a law. All one can say is that laws are more systematically related to one another than are accidental generalizations.

To analyze theories as collections of systematically related lawlike statements has seemed unsatisfactory to many philosophers. The logical positivists were particularly worried about the fact that propositions in scientific theories often appear to refer to things that cannot be observed. How can one have good reason to believe such claims? How indeed can one even understand what such claims mean? The logical positivists sharpened these questions and made the notion of a "systematic relationship" precise, by arguing that theories form deductive systems. Theories are primarily "syntactic" objects, whose terms and claims are supposedly interpreted by means of "correspondence" rules.¹⁵ Let me explain.

At the end of the nineteenth and the beginning of the twentieth century, dramatic breakthroughs were made in formal logic. In particular it was clearly appreciated that deducibility could be construed as a *formal* relationship that was entirely independent of the *meanings*. For example, one can infer the sentence r from the sentences s and "if s then r " without knowing anything about what the sentences s or r assert. Logicians explored the possibility of constructing formal languages in which logical relations would be precise and in which the ambiguities of ordinary languages would be eliminated. In order to say anything with a language, the language must have meaning, but the idea was to study the syntactic properties of language and its meaning or "semantics" separately.

The logical positivists believed that scientific theories should be expressible in one of the formal languages developed by logicians. From the

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axioms of the theory all theorems would follow purely formally (just as r follows from s and “if s then r ”). For the theory to tell us about the world, it needs an “interpretation”: We must be told what its terms and assertions mean. “Correspondence rules” are statements that are both supposed to provide that interpretation and to permit the now-interpreted claims of theories to be tested. Originally correspondence rules were conceived of as explicit definitions of each term in the theory, but the positivists soon realized that the relationship between theory and observation (to be discussed in the next section) is more complicated.

Few contemporary philosophers still accept the positivist view of scientific theories. Theories cannot be formalized in the way in which the logical positivists wished, and to view scientific theories as primarily syntactic objects does not do justice to the way in which theories are constructed or used. Furthermore, the problems of relating theory to observation, in the form in which the positivists posed them, are intractable. Many philosophers now settle for an informal construal of theories as collections of lawlike *statements* (not uninterpreted, purely syntactic sentences) systematically related to one another. But I should mention one other contemporary alternative that helps one to understand the nature of theoretical *models* in economics.¹⁶

Theories might be regarded not as sentences but as predicates or concepts or as definitions of such predicates or concepts.¹⁷ A theory such as Newton’s theory of motion and gravitation on this view does not make assertions about the world at all! Instead it is merely a predicate such as “is a Newtonian gravitational system” or a definition of such a predicate. Of course, scientists still make claims about the world. They do so by *employing* theories, by asserting that the predicates that theories constitute or define are true or false of systems of things in the world.

Drastically oversimplifying, the predicate view of theories maintains that instead of offering theories such as, “All bodies in the universe attract one another with a gravitational force and . . .” scientists offer “theories” such as “Something is Newtonian system if and only if all bodies in it attract one another with a gravitational force and . . .,” and then use such “theories” to make empirical claims such as “The universe is a Newtonian system.” From such empirical claims one can, of course, then deduce that “All bodies in the universe attract one another with a gravitational force and . . .” The predicate view thus appears to be simply a detour. Why should one want to take it?

There are several reasons. The predicate view possesses important formal virtues. It is easier to reconstruct the claims of science in a rigorous mathematical way if one employs the predicate view. It is more important for our purposes that the predicate view offers a useful way to schematize

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the *two* kinds of achievements involved in constructing a scientific theory. One kind of achievement is evident: A theory must identify regularities in the world. But science does not proceed primarily by spotting correlations among known properties of things. An absolutely crucial step is constructing new concepts – new ways of classifying and describing phenomena. Much of scientific theorizing develops such new concepts, relates them to other concepts, and explores their implications.

This kind of endeavor is particularly prominent in economics, where theorists devote a great deal of effort to exploring the implications of perfect rationality, perfect information, and perfect competition. These explorations, which are separate from questions of application and assessment, are, I believe, what economists (but *not* econometricians) call “models.” One can thus make good use of the predicate view to help understand theoretical models in economics. Whether one calls these models “models” or “theories” is largely a matter of terminology. I prefer to reserve the word “theory” for a set of general assertions about the world.

Theory and observation

The logical positivists were firm empiricists in two different senses. They were empiricists about *assessment* in science: Sensory experiences – the results of observations and experiments – constitute the ultimate evidence for or against consistent claims about the world (“synthetic claims”). And they were empiricists about *meanings*: To be able to understand a concept or term (except for purely logical or mathematical terms), one must be able to relate that term somehow to sensory experience. Because they were empiricists in both senses, the logical positivists found “theoretical claims” – that is, sentences that purported to talk about unobservable things or properties – doubly problematic. How could such theoretical sentences be tested? How could such theoretical sentences have any meaning at all?¹⁸

At first some of the logical positivists hoped that explicit biconditional truth conditions for such theoretical sentences might be found, that is, that one might be able to state that a theoretical sentence, *T*, was true if and only if some nontheoretical sentence (some sentence talking only about observables), *O*, were true. In fact, it was even hoped that explicit definitions might be given for each of the terms occurring in such theoretical sentences. “Operationism” (or “operationalism”), the view that all terms in science must be defined individually in terms of some measuring or checking operation, can be regarded as a variant of such an early and overly restrictive positivism. Given the sort of logic that the logical positiv-